other times his language is rather singular, as when he says that stammering "frequently arises from a muscular defect, giving rise to a *clumsiness in getting the tongue round one or more letters*" (p. 94), or speaks of the vocal bands being "tensed" (p. 105), or says that "musical gifts of voice are rather *phylogenetic* in their origin," the word in italics not appearing even in Mayne, or speaks of "living up hills" (p. 146). In a book written for singers and public speakers Latin and Greek and technical expressions should certainly be explained, if not avoided, such as *phylogenetic*, already adduced, and *frænum linguæ* (p. 95). The article on hygiene conveys a good deal of information, but we suspect most readers will rather remember the amusing account of the dietetic habits of singers, quoted from other sources, on p. 114, than be able to dig out what relates to the voice from the great mass of other matter. In conclusion, we cannot help feeling that the words "the science of," in the title, are not justified by the book itself, and might be advantageously replaced by the single word "on."

Ceylon Coffee Soils and Manures: a Report to the Ceylon Coffee Planters' Association. By John Hughes. (London: Straker Bros. and Co., 1879.)

THE writer of this report has at least gathered together a large amount of useful information about the coffee plant, coffee soils, and coffee manures. As an agricultural chemist he has, not unnaturally, attributed excessive importance to the composition and condition of the soils in which healthy and diseased coffee trees are found; manures also are indicated as amongst the chief remedial measures. Doubtless, the proper maintenance of the "condition," as it is technically termed, of coffee soils has been woefully neglected. Indeed, where there is neither rotation nor even alternation of crops the difficulty of securing continued vigour of growth and ample crops of fruit must be considerable, even when soils are rich and seasons favourable. But let any adverse influences, whether of excessive rainfall, or of mechanical and chemical injury to the soil occur, and then the plant is more likely to succumb to the attacks of its enemies, vegetable and animal. Thus wheat straw deprived of adequate supplies of soluble silica becomes more subject to injury from insects and mildew. Other examples might be found of a connection between certain deficiencies in the soil and certain diseases in the plant, but it is unsafe to make a hasty generalisation on this point. In combating the coffee-leaf disease we must first of all devote ourselves to the fungus which is its direct cause. There can be little doubt that calcium sulphide, which proved so efficient a means of destroying the *Oidium* of the vine will be equally destructive to the *Hemileia vastatrix*. A mixture of sulphur and quicklime, or a wash made by simply boiling these two materials together, is much less active.

When Mr. Hughes makes suggestions about the sources of manurial substances available for Ceylon, about the making and preservation of cattle and vegetable manure, and about terracing and draining, we can heartily endorse his recommendations. And when he gives us a number of careful analyses, some of which are of considerable interest, even apart from their connection with the growth of coffee, we are grateful for information which is sure to become useful under some circumstances and at some time. But there are certain portions of Mr. Hughes's Report which seem to have been introduced with no special object, or which are of questionable value. We hardly need to be taught that "Planters want a practical remedy rather than an elaborate description of the disease" (p. 140). The appearance of what look like recommendations of the manurial preparations of particular manufacturers should have been avoided (pp. 27 to 30). We could have spared the repetition of the well-worn table of manurial values on p. 100, and the analysis of Bude sand (p. 36). The term *granitic* as applied (p. 37) to a limestone containing over 70 per cent. of calcium and magnesium carbonates needs a word of explanation. Of really interesting data furnished by Mr. Hughes we may cite the analyses of castor-seed cakes (p. 15), in which the nitrogen is shown to differ widely—brown and black cakes containing but $4\frac{1}{2}$ per cent., while white cakes show no less than $7\frac{1}{2}$. Although we do not believe in the third decimal places (how often can we chemists secure accuracy in the tenths?) in Mr. Hughes's soil analyses (pp. 46, 53, 65, 72, 77, 81, 150)—particularly as his phosphoric acid determinations were not made by the molybdic acid process—yet these results represent a mass of laborious researches, and ought to furnish much material for the management of Ceylon coffee soils. The analyses of healthy and diseased coffee-leaves (pp. 142-144) deserve careful study; they point unmistakably to the fungoid origin of the disease.

A. H. C.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Cloud Classification

BETWEEN M. Poëy and his latest critic (NATURE, vol. xxii. p. 96) it would be impertinent for me to interfere. But until my objection to a part of Howard's original classification has met with some response from those who maintain the adequacy of that classification I must continue, at the risk of some repetition, to call attention to this objection. It can be stated briefly, and I do not see why the answer thereto, if such exist, should be deferred as too long for discussion.

E. H. appears to admit that an observer, when within a cloud (which is then to him a fog), cannot distinguish *cumulus* from *stratus*. He, however, elevates the stratus, or rather one variety of it, "a few feet (or even inches) from the earth," so as to cut "the taller trees in a horizontal line, leaving their tops and bottoms free." He proposes to shelve the question "whether it is desirable to use the term 'stratus' for clouds in a totally different sky region, which differ both in their origin and their nature from the true stratus" (*i.e.*, from the *stratus* of the sky-region of half-length elms in the Thames basin). Now it is precisely this question which the large and growing class of observers, who wish to record the modifications of clouds, can no longer permit to be left unsettled. If clouds are to be classified according to their form at all, some name is absolutely required for a class of clouds which is in all latitudes common, and in the higher predominant. These are the clouds to which the observers can neither give the title *cumulus* nor the title *cirrus*, the clouds which are disposed in beds or layers whose vertical thickness is small. When in trifling amount they arrange themselves in irregular disks or patches capable of being occasionally mistaken for *cumulus* when in the zenith, but elsewhere seen as streaks or threads transverse to the meridian. When in large amount they cover a great portion or the whole of the sky with a shallow and nearly level canopy. In England, putting together observations made at all hours of day and night, clouds belonging to this class are recorded in about 60 per cent. of the observations. Of observations made at 2 p.m. they occur in about 38 per cent.; of observations made between sunset and sunrise in upwards of 90 per cent. Of observations made at all hours in the English Midlands from October, 1879, to February, 1880, inclusive, they were recorded in 83 per cent. To leave this class nameless is intolerable. To give to them either of the compound titles *cumulo-stratus* and *strato-cumulus* is objectionable, because in form they do not resemble *cumulus* at all (I might add that to a defender of Howard's unamended system they also do not resemble *stratus* at all, differing, as we are told, not only in elevation, but in "origin and nature," both from ground-fog and middle-tree fog). A similar objection lies against the application to these clouds of

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the terms *cirro-stratus* and *strato-cirrus*. Finally the terms *cumulo-stratus* and *cirro-stratus* are sorely needed for varieties of clouds intermediate between the class I have described and the *cumulus* and *cirrus* types, if any part of Howard's terminology is to be left to us at all.

It would be a pity that that terminology, lucid and expressive, should perish, merely because, to a few minds, the originator of a system must needs appear infallible, and his classification perfect as Minerva when issuing from the head of Jupiter. I think that Luke Howard would have been the last to put forward such a claim. W. CLEMENT LEY

June 8

The Motion of Fluids

PROF. REYNOLDS, in the course of his review (NATURE, vol. xxi. p. 342) of my book on the above subject, cites two instances in which I have been guilty of what he considers loose and vague reasoning. I would ask space for a few remarks on the points in question.

To take the more important matter first, Prof. Reynolds says, *apropos* of a certain proof of the velocity-potential theorem given in Art. 23:—

"Mr. Lamb has offered a proof of this now historic theorem, which, if judged by the space it occupies, should be much simpler than the acknowledged proofs of Cauchy and Stokes. As no authority is cited, it would appear that this proof is here given for the first time. If so, the author has done himself great injustice in not examining or explaining his reasoning more closely. For, as it stands, it suggests the idea that he has ignored the fact that dx, dy, dz, on the left of his equation, are integrals through a finite time, and hence, inasmuch as he has given no reason to the contrary, may be of a different order of magnitude from their initial values, da, db, dc, which appear on the right of his equation. If this is not so it is a peculiarity of the motion of continuous fluid, and needs establishing; otherwise we might infer that two people who had once shaken hands could never after be so much as a mile apart."

Prof. Reynolds, who himself strongly recommends the careful study of "work from the master's hand," will hardly take it amiss if I ask him to turn to the proofs which he justly cites as classical, and to notice that they contain, one of them (Cauchy's) in exactly the same form, the other in a form which is mathematically equivalent, the very assumption which he here calls in question. The assumption is in fact nothing more than a tacit limitation, which is made at the very outset of the subject, as to the class of motions which are proposed for study. In the "Eulerian" method it is implied that the first derivatives of the component velocities u, v, w with respect to the co-ordinates x, y, z are to be everywhere and always finite throughout the motion considered; in the "Lagrangian" method the corresponding, and equivalent, assumption is that the derivatives $\frac{dx}{da}, \frac{dx}{db}, \frac{dx}{dc}$, &c, and also $\frac{d^2x}{da\,dt}, \frac{d^2x}{db\,dt}, \frac{d^2x}{dc\,dt}$, &c., are to be finite. We do not assert that these are universal characteristics of fluid motion, for it is easy to imagine cases in which they are violated; we merely exclude such cases *ab initio* from the scope of our investigations. But, in one form or another, these fundamental limitations are, from the point of view of analytical hydrodynamics, unavoidable; they are made implicitly every time we write down the equations of motion, and it is therefore not surprising that they should be found to be essentially involved, not only in the proof which Prof. Reynolds on this account criticises, but in every other proof of the velocity-potential theorem which has yet been propounded.

I have only to add that the proof in question is, and professes to be, merely a very obvious corollary to H. Weber's transformation of the Lagrangian equations.

The other passage of Prof. Reynolds's review which I wish to notice is as follows:—

"There is a considerable amount of vagueness attending the author's use of the term *particle*. Having rightly defined fluids as being such 'that the properties of the smallest portions into which we can conceive them divided are the same as those of the substance in bulk,' he proceeds to reason about a particle as though it were a discrete quantity, the position of which is defined by some point, thus ignoring the fact that, according to his definition, the same particle of fluid may at one time be a sphere, at another a filament of indefinite length, or a sheet of indefinite breadth. This vagueness appears to have led him into error in Art. 11."

A good deal of this criticism is, I think, met by the remarks already made. In a fluid moving subject to the conditions I have stated, only finite changes of shape can be produced in a moving element within a finite time.

Prof. Reynolds does not indicate the precise nature of the "error" which he finds in Art. 11. After a careful reconsideration, the argument of that article appears to me to be sound; but I am free to confess that it is not stated with all the clearness desirable, and that the article is further disfigured by an unfortunate clerical error in the foot-note, where "$u = \pm\sqrt{x}$" should be read for "$u = \pm x$." HORACE LAMB

Adelaide, March 30

On the Physical Aspects of the Vortex-Atom Theory

WILL any charitable person explain a difficulty which I (and other non-mathematical people) have encountered when seeking to understand and be satisfied with this theory?

The only proof of those properties of vortex rings which match the physical properties of atoms that I have met with is that in Besant's "Hydromechanics"; and is based on the initial-co-ordinate method.

Now it seems to me that this method assumes what is equivalent to the permanence of the vortex filament; so that in proving the latter by use of this system of co-ordinates we may be merely arguing in a circle.

For it assumes that if initially we have any infinitesimal tetrahedron $\delta\alpha . \delta\beta . \delta\gamma$, then after the *finite time*, t, this will still form a tetrahedron $\delta x . \delta y . \delta z$.

Now I cannot see that one can assume this; that—to use the words in a late article of NATURE—"if two people have once shaken hands they can never be 100 miles apart."

And this inseparability of the particles of a fluid thus *assumed* bears a very close relation to the permanence of the vortex filament which we wish to prove. W. L.

Cheltenham, May 29

[It appears to us that our correspondent here confuses between the permanence of any fluid filament and the permanence of the vortex character of the filament. The assumption that every filament remains continuous cannot be said to be equivalent to assuming that the direction of the filament at every point remains coincident with the axis of rotation of its constituent elements at that point, which is what Helmholtz has taught us.—ED.]

The Aurora Borealis and its Colours

WITH regard to Drs. De La Rue and Müller's paper on the Aurora (NATURE, vol. xxii. p. 33) there is still a point I should like to see explained. Is it considered by physicists that in electric discharges similarity of colour is sufficient to indicate similarity of constitution, even when their spectra are quite unlike? The paper, together with the reply to Prof. Smyth, certainly seems to imply this; though I have not previously seen it stated to be the case.

With regard to the red part of auroræ, so far as my observations indicate its position, they show it to be above the greenish part in the auroræ seen here; though according to Weyprecht's observations, it is below the green in the Arctic regions.

Sunderland, June 9 T. W. BACKHOUSE

A New Audiphone

FURTHER experiments on the timbre of musical instruments as rendered by the audiphone have led me to the selection of the following as a distinct improvement on the birchwood veneer, both for musical purposes and also for ordinary conversation. It has the same advantage as my previous form in not requiring to be held by the hand, it costs nothing, and requires no making. Take a sheet of stiff brown paper about 11 × 15 inches, the paper being such as is ordinarily used for making up heavy parcels. Put the ends together, the middle forming a loop, and hold the ends between the teeth. The paper must be pretty stiff, as the loop must stand out round and full, and of course the paper must be without folds or creases. THOS. FLETCHER

Museum Street, Warrington

Crystal-Ice

IN reference to the "crystal ice" proposed by Dr. Calantarients, of Scarborough, for skating upon with ordinary skates,

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